It will be noted that the acrospores are comparatively uniform in size, and are wholly different from the extremely variable brown Fungus-germs produced from the Zooglœa masses.

What has just been illustrated is only one of the ways in which Fungus-germs are produced in the pellicle from Zooglœa masses. Anyone working at this subject will have no difficulty in recognising many other modes in which they originate. Sometimes the germs separate from the Zooglœa masses as colourless units, and then take on an almost black colour before they begin to germinate, as in the specimen shown in Fig. 12, which was taken on the twelfth day from another pellicle on a hay infusion.

I have frequently found that these heterogenetic Fungus-germs are small ovoid bodies with one, or sometimes two, nuclear particles such as may be seen in this case, and also in some of the small brown units shown in Fig. 9. It is interesting, moreover, to find that the immediate products of segmentation which are about to develop into flagellate Monads present, except for their spherical shape, very similar characters, as may be seen by reference to Fig. 5, A.

It seems to me impossible to doubt that we have in the processes which I have just described definite instances of heterogenesis. The fact of the individualisation and the segmentation of these Zooglœa masses cannot be denied. It is plain, indeed, that from such aggregates of bacteria, by common consent regarded as belonging to the vegetal kingdom, we have the production of typical animal organisms, and that, as I have said, no kinship between

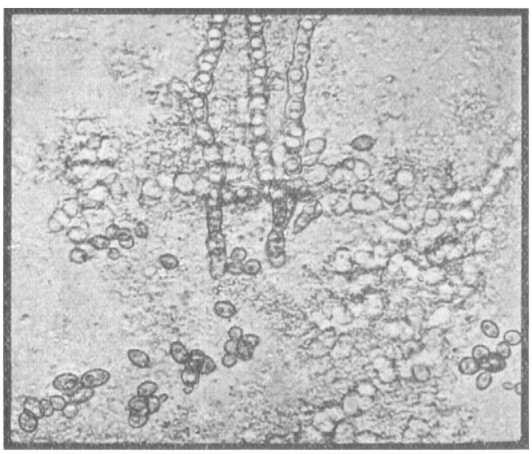

FIG. 12.—Heterogenetic Fungus germs becoming black and germinating (×500).

bacteria and flagellate Monads has ever been recognised, or even suspected, by the great majority of biologists; and, though it cannot be said that there is the same lack of kinship between bacteria and Moulds, it can certainly be said that the majority of biologists have never suspected any such relation between these two forms of life as that which has now been made known.

I care little what names may be given to the bacteria, though I am certain that many different varieties are prone to form zooglœal aggregates, and to go through one or other variety of such changes as have just been described. Being much interested with these processes that go on in nature, and under more or less natural conditions, I have been familiar with such phenomena for more than a generation[1]; but although they were made known so long ago I am not aware that any bacteriologist in Europe, America or elsewhere has ever repeated my observations. Bacteriologists to whom I have personally mentioned the subject have, with only one exception, shown not the least desire to examine specimens or to follow up the inquiry. They seem wedded to their strict laboratory methods, and seemingly prefer to have dealings with nothing but pure cultures and sterilised media. I do not deny for a moment the enormous increase of knowledge, and the benefit which has accrued to the human race, from their studies, but should like to see a little more toleration displayed for those who prefer to work in a different way, and strive to find out what goes on under more natural conditions—undeterred by the much talked of but much over-rated risk of "infection." Assuredly, in the future, much of what is now ascribed to "infection" will be differently regarded as the "origin of species" by heterogenesis becomes more and more known.

If such processes as have just been described are continually going on in nature, but are not to be met with in the laboratories of bacteriologists, it should make us hesitate to repudiate a natural origin of living matter at the present day simply because undoubted proof of its occurrence cannot be produced by laboratory experiments. If it occurred in the past the law of Continuity would lead us to expect that it has been continually occurring ever since, and, as I said in my letter of November 10, "if the origin of living matter takes place by the generation in suitable fluids of the minutest particles gradually appearing from the region of the invisible, such a process may be occurring everywhere in nature's laboratories, though altogether beyond the ken of man."

H. CHARLTON BASTIAN.

[1] See *Proceedings* of the Royal Society, 1872, vol. xx. p. 239.

The Temperature of Meteorites.

DURING the early part of the year 1901, when I was on the staff of the Elswick Works, it occurred to me that it would be useful and interesting if a connection could be made between the conditions of the flight of artillery shells and of meteorites. Later in the same year I made a preliminary mathematical investigation into the matter, and as a result a paper on the temperature of meteorites was sent in as an essay to compete for the Smith prizes at Cambridge. It was distinguished from other essays sent up in not receiving a prize.

It has since remained a strong wish on my part some day to work up the subject into a form fit for presentation to a scientific society, but the pressure of other matters has prevented this. In order, therefore, to preserve at least its outlines, I give here a brief exposition of the premises, the procedure, and the conclusions of the essay.

Ordinary ballistic tables contain a wealth of information as to the retardation experienced by projectiles of all sizes and of one general shape. The shape of the shell is well known. If the same rules can be made to fit the motion of meteorites it is clear that the velocity at any time can be obtained, and thence the loss of energy due to the obstruction caused by the air. This energy reappears as heat, sound, electrical energy, chemical energy, &c. Of these by far the most important is heat. Thus the conditions under which a meteorite "heats up" can be ascertained, and if it be assumed that all the energy is so spent, it is obvious that a superior limit to the resulting temperature may be obtained. One further point should, however, be mentioned—a meteor which reaches the earth is called a "meteorite," and the velocity necessary for this is such that the time of passage through the material part of the earth's atmosphere is so short, say five seconds, that chemical burning will not, in general, introduce any sensible error. Such error as might be introduced would be of the opposite sense to radiation losses, themselves small for much the same reason.

Meteorites may be of almost any shape. I have only considered the shell shape, as it is the only one the flight of which has been thoroughly investigated by exhaustive experiment.

According to Ingall's "Exterior Ballistics," the law of the resistance of the air is a function of the velocity which, for velocities above 1380 feet per second, is the velocity squared. For meteoric problems, velocities less than this are unimportant. Whether this simple law would hold good for velocities of, say, 20 miles a second, or even the 7 miles a second which the earth can impose, is not known, but for lack of a better it has been necessary to employ it.

The next difficulty, and of difficulties there is no small number, lies in the varying density of the air. A few thousand feet is the upward limit of ordinary projectiles. Even for howitzer shell the correction for rarefaction is so slight that the simplest kind of correction is enough. For

meteorites, however, more extended treatment is required. I have taken the resistance to be in direct proportion to the density of the air. To do even this requires a knowledge of the density at all altitudes, and for this I have assumed an isothermal distribution of temperature. The theory of adiabatic distribution makes the atmosphere cease at distances well within twilight and meteor phenomena, and is therefore of no use. Probably something between these two would be most accurate, but its precise form is not of great importance in this investigation owing to the very slight influence of the uppermost reaches of the air on the motion of meteorites.

I now come to the meteorites themselves. Many sizes have been considered, but chiefly diameters of 0·10 inch and 12·0 inches. I refer to these as the "small" and the "large" meteorites. When other sizes are mentioned their diameters are given. I have further taken two materials, viz. iron and stone (trap rock), representing holosiderites and asiderites. The thermal constants for the materials are those found by Forbes.

I stated above the circumstances in which a knowledge of the heat energy given to the meteorite might be taken to be known. To find the temperature distribution in the *interior* of the iron or stone I have adopted the approximation of considering the meteorite to be cylindrical, and then utilising ordinary cylindrical coordinates. During the investigation a good many results were obtained which indicated methods by which the simple labour of the work could be lightened. Some of the more cumbersome expressions could be simplified by dividing the distance between the earth's surface and infinity into two regions, that within the sensible effect of the atmosphere and that without.

Many results were obtained during the investigation. In the large meteorite it was found that for all velocities of approach the temperature at the centre was a most minute fraction of that at the surface. For the small meteorite it was found that the final velocity was always very small and the time of flight correspondingly great, with the result that the whole of the material would be consumed before reaching the earth's surface—this would then properly be termed a meteor, not a meteorite. In its turn this consideration gives the altitude at which incandescence would occur. The small iron meteor would burst into brilliance at 45 miles up, and the stone one at 68 miles. To obtain a superior limit to the point of incandescence I assumed a meteor the diameter of which was only a millionth of an inch. For iron, brilliancy is obtained at 106 miles, and for stone at 129 miles. These figures are obtained by assuming the meteors to have the maximum velocity which the earth could impose. If, however, an initial velocity of 250 miles per second be assumed, surely a superior limit, incandescence would occur some 35 or 40 miles further off, so that the greatest height for visibility would lie well within some 170 miles.

An iron meteorite 3 inches in diameter falling to the earth from an infinite distance would begin to get warm about nine seconds before reaching the earth, and continue to increase in temperature for about seven seconds, after which its velocity would be practically "killed," and two seconds later it would reach the earth at about two-thirds of a mile per second. This represents a typical case for what might be termed the "twelve pound shell" size.

In the "twelve pound shell" size the internal temperature falls off very rapidly towards the interior. Thus, taking the mean temperature in the severest case as 1·00, the surface temperature was 2·2, and at a depth equal to a fifth of the radius (0·30 inch) the temperature was about 0·3 only, whilst at the centre it was 0·0016. So that for the most excessive surface temperatures the central temperature would be well below the temperature of liquid air, assuming, of course, that the initial temperature of the meteorite is at the absolute zero.

The steepness of the heat gradient at or near the surface is the probable cause of the nodular appearance of meteorites. Great resistance to the inward flow of heat would be offered by any internal veining, and as a result such surfaces of separation would tend to become the limiting surfaces for any burning which might occur.

The various formulæ used to obtain the above results were suited to a subsidiary investigation, viz. that of the problems connected with the ejection of rock from terrestrial volcanoes. The results of such an investigation may be briefly summarised as follows:—Had the earth no atmosphere all masses shot off vertically at 7 miles a second and over would fail to return. With the existing atmosphere the large meteorite would require a velocity of 13 miles per second, and the "twelve pound shell" would want a velocity of 78 miles per second. These velocities are not without interest in view of the theory that meteorites originated from terrestrial volcanoes. Smaller velocities would suffice were the masses discharged from high altitudes. Thus, from a height of 5 miles, the velocity for the large iron meteorite would be only $8\frac{3}{4}$ miles per second, and for the "twelve pound shell" only 18 miles a second. Further calculation shows that with an initial velocity of 7 miles a second the large meteorite would rise to only some 120 miles, and the "twelve pound shell" to between 40 and 50 miles, and both would then fall back to the earth.

In conclusion, the result of the investigation may be said to have created a strong presumption in favour of the following general deductions:—

(*a*) That the velocities of meteorites are materially changed by the resistance of the atmosphere, and, in general, by a fractional part of the velocity which is independent of the velocity of approach.

(*b*) That the superior limit for incandescence is probably about 150 miles above the earth's surface.

(*c*) That no iron meteor the original weight of which was less than 10 to 20 lb. reaches the earth's surface, and that when a meteor does do so the temperature of its centre is not in general above that of liquid air (assuming the temperature of space to be zero).

I am aware that the whole structure of the investigation rests on the evil principle of extrapolation, but until man is capable of experimenting with velocities of 10 or 20 miles a second, and surviving thereafter to record his results, no other manner of investigation seems possible.

London, November 13. H. E. WIMPERIS.

Mount Everest: the Story of a Controversy.

I HAVE read with interest in your columns under this title a carefully compiled and instructive account of the discussions that have from time to time during the past fifty years broken out with regard to the naming of the highest measured point on the earth's surface, Peak XV of the Indian Survey.

I have long maintained it to be a matter for regret that the monarch of mountains should be called after any individual, however eminent, and I am still of this opinion, which is shared by most mountaineers and mountain lovers. We should prefer that Peak XV should bear a Nepalese or a Tibetan name, even had one to be invented for it, as twenty years ago Alpine Clubmen, in accord with Russian surveyors, found or invented native names for many of the great peaks of the Caucasus.

But, since your correspondent appeals to me not to prolong the controversy further, I must remind him that the opinion I have expressed is an individual and not an official opinion. For ten years I have had no official connection with the Royal Geographical Society.

Should the council of that body resolve that, considering the length of time the title "Mount Everest" has been more or less in use in this country for Peak XV, the absence of any evidence that that individual peak is designated as, or included in the designation of, Gaurisankar by the Nepalese, and the practical inconvenience (whether the name be authentic or not) of introducing a new Tibetan name such as Chomo- or Jamokangkar, it is expedient that the title Mount Everest should be generally accepted, I shall acquiesce. For I attach greater importance to the general principle than to the particular case, and I believe the protracted discussion and many protests summarised in your columns have served their purpose in helping to discourage the practice of giving personal names to mountains.

I should add that foreign geographers are not, as your correspondent suggests, mainly dependent on the *Geographical Journal* for information in this matter. Captain Wood's report has been noticed in that well known periodical *Petermann's Mitteilungen*.

DOUGLAS W. FRESHFIELD.